

RESPONSE TO COMMENTATORS

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My thanks to John Donahoe, Charles Shimp, and John Malone who have commented so thoughtfully on Baum's review of *The New Behaviorism* (TNB) and my response to it. So as not to prolong a discussion that is already approaching book length, I will restrict my final remarks to just three questions: Have I misunderstood Skinner's position on theory and the social implications of behavior analysis, or was he simply wrong? Is my idea of internal state nonbehavioristic or even (perish the thought!) cognitivistic? Is the distinction between molar and molecular behaviorism a paradigm conflict of the sort made famous by Thomas Kuhn?

Donahoe comments ". . . to believe that a statement of Skinner could be obviously wrong-headed about such an important matter [as the role of theory in science] is to believe that Skinner was either careless or stupid" (p. 85). But equally incredible, surely, is the assertion that someone who studied under Skinner and has read most of his work has totally misunderstood him. In other words, the argument that one of us must be an idiot—Skinner or I—won't wash. Even Homer nods, and many a great man has made a great mistake. Everyone is fallible, even the parties to this debate.

I am not the only one to interpret Skinner as fundamentally antitheory. Laurence Smith has collected a whole litany of antitheory comments by Skinner, and I quote them in TNB. They sit ill with Skinner's admission elsewhere that behavior analysis admits of theories that are "a formal representation of the data reduced to a minimal number of terms" (1950, p. 69). The present debate is clear evidence that behavior analysis itself has largely rejected any theory that hypothesizes hidden variables.

Donahoe points out the real problem (and Malone, by noting how attractive molar behaviorism is to students, seems to agree with him). Skinner had conflicting aims: First (though later in his career), he wanted to build a field—a movement—that would change society. This is what Donahoe terms his "pragmatic/political" aim (p. 86). For this, simplicity was essential. Second (during his early career), he wanted to found a new science. Donahoe terms this his "principled" aim (p. 86). In other words, Skinner was not totally consistent. On the one hand, he lamented the demise of the cumulative record and the possibilities of moment-by-moment analysis that it offered ("Farewell My Lovely!", Skinner, 1976). But on the other, his consistent antagonism to any theory adequate to deal with dynamics (which must involve some hidden variables) left functional analysis—"laws" like Weber's law or the matching law—as the only acceptable alternatives. This is why operant theory for years has been dominated by (I almost wrote "stuck at the level of") molar laws. This development was not, as Skinner complained in his ". . . Lovely!" article, a reaction against his ideas, but was in fact the only path he left open. After all, if all theory that "appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1950, p. 193) is prohibited, but we want to explain things anyway, then molar "laws" are all that is left. Skinner was unworried by the fact that his proscription would have ruled out most of the great theoretical developments in physics and biology, from the atomic theory and the theory of the circulation of the blood through genetics and the wave theory of light. Almost every important theoretical advance in science has postulated "events taking place somewhere else [or] at some other level of observation." Donahoe is quite right to insist on the necessity for real-time theory—but wrong to credit

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Skinner with sympathetic anticipation of his proposal. Far from promoting the solution, Skinner was part of the problem. (Donahoe is also right that Morgan, at least, could not accept the gene until genes, or chromosomes, had actually been observed. But without the postulation of genes “at some other level of observation” no one would have gone looking for them; likewise, atoms and the synapse.)

Incidentally, I believe that Donahoe is wrong in citing reinforcement theories such as Premack's, Allison's (or the generalization of those two in the form of the minimum-distance model: see Staddon, 2003, chap. 7, for a recent review),—or even the Rescorla-Wagner model—as consistent with Skinner's prohibition of “events taking place somewhere else, at some other level of observation . . .” The minimum-distance model postulates a behavior space, and relative response costs, that are not directly observed, and the Rescorla-Wagner equations contain terms, such as λ , the maximum associative value, that cannot be measured directly.

The distinction between early and late Skinner resurfaces in connection with his views on behavior analysis and society. Donahoe quotes an admirable early statement: “. . . it is a serious, though common, mistake to allow questions of ultimate application to influence the development of a systematic science at an early stage. . . . The book [*The Behavior of Organisms*] represents nothing more than an experimental analysis of a representative sample of behavior. Let him extrapolate who will (1938, pp. 441–442)” (p. 90). But Skinner threw caution to the winds later and went beyond not only the facts and our state of knowledge, but beyond the limits of science itself. Science cannot decide all issues. It can tell us that one course of action is more likely than another to lead to a desired outcome. But it can not ultimately say what that outcome *should* be—even if we accept Skinner's version of the naturalistic fallacy, that the “survival of a culture” is the ultimate good (this argument is made in *TNB* and, in more detail, in Staddon, 2004). It is simply not sufficient to dismiss questions of ultimate value by saying “To confuse and delay the improvement of cultural practices by quibbling about the word *improve* is itself not a useful practice” (Skinner, 1961, p. 6). Nor will it do to

discount our very limited ability to predict societal outcomes by saying (Donahoe's quotation) “Much of the argument goes beyond the established facts. I am concerned with interpretation rather than prediction and control” (Skinner, 1974, p. 21). “Our knowledge . . . is limited by accessibility, not by the nature of the facts. . . . As in other sciences, we often lack the information necessary for prediction and control and must be satisfied with interpretation, but our interpretations will have the support of the prediction and control which have been possible under other conditions” (Skinner, 1974, p. 194). Skinner here is doing no more than arguing by analogy. To base major recommendations about teaching practice, parenting and the disciplining of children on such an argument is little short of preposterous. Shimp doesn't discuss this issue, but I believe that Malone agrees with my reservations about Skinner's extrapolations to society.

Although Donahoe accepts the idea of hidden variables (a better term, perhaps, than “internal states,” which seems to induce Pavlovian reactions in many of my colleagues) in general, he objects if they are inferred solely from behavior because “Behavioral observations, by themselves, insufficiently constrain intervening variables because a given environment-behavior relation can be produced by any of a large number of underlying processes” (p. 88).

There are two problems with this position. If we limit “intervening variables” to those directly measurable physiologically then of course they are no longer hidden. They are external with respect to the system under study. The idea of hidden variables cannot be saved by restricting them to measurable physiology. The second objection, that behavior “insufficiently constrains” our models, is also not conclusive, because this objection applies to *all* scientific theories. It is well known that any finite set of facts is consistent with an infinite number of possible theories. This problem is easily dealt with by *prediction*. Darwin knew what he was doing when he wrote in his notebook: “The line of argument often pursued throughout my theory is to establish a point as a probability by induction and to apply it as hypotheses to other points and see whether it will solve them” (Darwin, 1838/1987).

Donahoe's discussion of my model for rate-sensitive habituation illustrates this point and another one. The model was devised to account for data from simple experiments in which recovery from habituation was measured after different histories, either closely or widely spaced stimuli. Donahoe correctly points out that the slower recovery after the widely spaced series is consistent with the principle of stimulus generalization. But that principle—and the whole notion of “context”—is too vague to predict what will happen if we switch stimulus spacing within a session or present stimuli with varying interstimulus intervals or with different absolute values but the same ratio, and so on. A real-time model such as the multiple-time-scale model can deal with all those “contexts.” But the two approaches, via generalization or a real-time model, are *not in conflict*. The model (if it is correct) provides an explanation for how generalization works in this situation; it is not in conflict with the principle.

Is the idea of internal state nonbehavioristic? No more so than the generic stimulus and response. If a response is a class determined by covariation with a stimulus class (Skinner, 1935; Staddon, 1967) then why should we not accept internal state as a class of histories defined by their convergence on the same class of future histories? Models are then nothing more than a way to summarize the effects of history. Instead of enumerating all possible habituation series, we can summarize the effects of any given history by the values of the state variables of the model, which will be (or should be—in Hull's case all too often they were not) much fewer in number than the number of histories to be summarized.

Finally, there is the issue of molar versus molecular behaviorism raised by Shimp. I believe that here also lurks a philosophical error, which is the attempt to prescribe in advance the proper form for correct theory. It can't be done! It is as if Robert Boyle had brandished his newfound Law and said “Only pressure, volume and temperature matter. All else is vain!” How on earth could he possibly know! Likewise, we cannot define for all future time just what behavioral measures will prove most useful. The proof is in the pudding: Which explanations—for contrast,

matching, schedule dynamics, and so on, and so on—are best? We can have only opinions on whether molar or molecular analyses will in the end prove most compelling; they are not (yet) matters of fact.

Moreover, many useful theories have both molar and molecular aspects. Consider, for example, a theory that Derick Davis and I advanced several years ago for the dynamics of choice (Davis, Staddon, Machado, & Palmer, 1993). Our aim was to explain both steady-state properties of choice, such as matching, and otherwise puzzling facts such as the increased rapidity with which equilibrium is reached when choices are switched more frequently (so-called reversal-learning set)—a result that reappeared under a different name several years later when the switching behavior on concurrent schedules was shown to be close to optimal under many conditions (Gallistel, Mark, King, & Latham, 2001). The *cumulative effects* model is very simple—you be the judge as to whether it is molar or molecular, or contains hidden variables. The idea was that the animal in a choice experiment simply accumulates the total number of reinforcers obtained for each choice response and divides that number by the total number of responses made. The higher the resulting ratio, the higher the value of that choice. When the animal must choose, it simply picks the response with the highest value. If it gets no reinforcement, then obviously the value of that choice falls, and with continued unreinforced responding, its value will fall below that for the other choice (which remains unchanged so long as it is not made) and the animal switches. There are some refinements to do with initial conditions (i.e., the initial values of the response and reinforcer counts), but that's basically it.

The model is in one sense about as molar as it can be. Every response and every reinforcer is counted. It is very different from *melioration*, Herrnstein and Vaughan's (1980) molecular-type model for the process. But the cumulative effects model is also molecular, because each choice is determined moment-by-moment by a winner-take-all rule. Yet it can explain both molar phenomena, such as steady-state matching, and molecular ones, such as the different rates of changing preference after different histories. The point is not whether this model is correct or not: It is

obviously too simple and makes no reference to time (nevertheless, it seems to capture something important about the way choice works on concurrent schedules). The point is that many interesting and useful models will have both molar and molecular aspects. It would be a pity, therefore, to rule out one or the other in advance.

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